THE COMPOSITION OF TAXONOMIC PAPERS.

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Taxonomy is the most important incident of Science—things must be named before we can write of them. If we consider the proportion of zoological papers at the present time, we find that about two thirds are systematic. But this proportion increases to seven-eighths if entomological papers alone are considered.

In taxonomy we usually speak of only two types of contributions: (*a*) text books, prepared for a mixed public, and (*b*) original work, which is intended for the entire scientific world, but whose chief appeal is directed to a small group of contributors. These form a special class, distinct in conception and treatment from other scientific papers.

The style of composition proper in a morphological, ontogenetical or phylogenetical paper would be found impracticable in a taxonomic paper. Yet, while the greater number of entomological papers are taxonomic, it nevertheless remains a curious fact that as yet no compendium for taxonomic composition has been published.

For papers other than taxonomic we have Dr. T. Clifford Albutt's excellent book, "Notes on the Composition of Scientific Papers" (MacMillan Co., New York, 1904, 8vo). While many of the chapters of this work would be of interest to systematists, they do not pertain directly to taxonomy; the volume, on the whole covers a quite different ground, that of scientific theses.

To say that nothing at all has been published on the present issues would invite criticism. On the contrary, I have found copious and more than sufficient material in the more prominent journals of the past two decades. But these contributions are widely scattered; moreover, they are written as protests and usually deal with a single topic only. While I cannot lay claim to originality in the suggestions included in this paper, I have endeavored to treat all of the more vital topics bound up with taxonomy; aiming to suggest such standards in writing as would conform to the various needs of those interested.

Briefly stated, a standard is the result of an average or consensus of opinions upon a given subject, hence a criterion. Thus far the only criterion of any worker has been the approval of his fellow-workers along the special line of work he has adopted. This basis is hardly sufficient, as not a small coterie of workers but the world at large is intended to be benefited.

It must be remembered that this paper does not treat of criterions of species, but with the composition of descriptions and general methods of presentation. That these are perfect and above reproach probably none will maintain.

I have talked over these matters repeatedly with scientific workers. Curious to say, dissatisfaction with present methods and with the absence of definite standards was prevalent everywhere. The necessity of co-operation toward the achievement of practical standards was sometimes very strongly expressed. A digest of all these opinions, private or published, may be summed up in the following: Better methods of description are desirable; the nomenclature of species and genera, of colors and types to be regulated; titles of articles to be made more comprehensive; reprints to contain place, time and name of publication, etc.

I. Standards for Descriptions.

A. Specific Description.—J——— s———, an unencumbered species: Front pale, palpi scaled, thorax with black stripes, antennae yellowish, abdomen spotted, legs with pale, wings with black markings. Head with short pile, abdomen tufted, a small tuft between the antennae, which are fuscous at the base, white toward the tips. Wings white, with four or seven black lines crossing them, the lines curved or straight. Abdomen with tufts black, exceeding anal angle. Legs long, with spurs. Palpi reaching the vertex, legs slightly darker at the joints.

I dare say, that no living man could determine a specimen from this extravaganza. Yet the description is made from an actual species—*Conchylodes platinalis*, Lepidoptera–Pyralidae.

Furthermore, it is typical of many descriptions of the past, and, I regret to say, of too many in the present days.

It is curious how little logic is often applied in formulating a description. One specialist, who has written hundreds of descriptions, seems to have found particular pride in making these as intricate and involved as possible; there is no logical sequence in the treatment of the main divisions; on the contrary, the acrobatic description jumps from antennae to legs, from abdomen to head, wings to palpi, venation to tarsi, morphological characters to vestiture, color to structure, etc., etc. So much so, that after comparison is completed one must begin over again, as it is impossible to remember the way through the labyrinth.

Descriptions should not be written for personal aggrandizement, but to announce a new fact or discovery to the scientific world. Such being the case, the description, once published, belongs to the world at large and no longer to the writer. The author therefore owes it to science that the facts of which the world is to become owner be presented in a manner most accessible to, and best applicable by other men. If the author for any reason whatsoever is careless and inaccurate he sins against science. After all, there is an intellectual as well as a moral conscience.

"Head pale, eyes small, dark, vestitute smooth and yellowish, body moderate, legs short, tibiae stout," applies equally well to Mr. Jones as to *Pediculus capitis* strolling on his head. Brevity may be the point of wit, but science is no joke; taxonomy deals with facts, not idiosyncracies. Who has not felt the bane of two to eight lined descriptions, any one of which harmonizes easily with half a dozen or more distinct species? I do not believe that an entomologist lives who has not at one time or other execrated these brief, vacillating descriptions. But why do entomologists continually write others that are no whit better or longer? Let it be known, that one thorough description covering three pages may be of more use and more valuable to science than three descriptions on one page. One may suggest that the perusal of brief descriptions saves time; but when we come to analytic comparison of closely related species the brief description forms an obstacle which results in considerable loss of time. What of genera whose species are extremely variable? Can the extent of specific variation together with a description of the aver-

age be summarized in twenty lines? Hardly. The ideal description will be a careful analysis of all body parts with all their appendages, attributes and characteristics, to be followed by a summary of salient characters of the type and a comparison to related species. I maintain that this cannot be accomplished on less than a page.

It is terrifying and discouraging to be confronted by a page of solid description, where all characters, whether head, thorax, abdomen, or wings, flow together in a solid phalanx, so that it is impossible to pick out readily any special point desired. Descriptions should be paragraphed or captioned. This costs no extra labor, and, in fact, presents a much neater appearance when published than the solid, uniform mass of words. Besides it affords greater facility to the student who wishes to look up certain characters for comparison.

Again, a description should not be isolated. I mean, comparison to related species and indication of the position of the new species should follow the description. It is reprehensible negligence to describe a new species from a genus already containing a dozen or more species and to omit all mention of either relations or position; such proceeding is indeed worthy of reprimand. To say the least, the work of the author will be placed in an extremely doubtful light. The thought suggests itself, that the author himself was ignorant of the relations and that he described a species at hap-hazard.

After all this, why pay any attention to identity, number, and custody of types? Why state the locality from which the types came? Why select a holotype from a series of twenty specimens that show considerable variation? No one is ever expected to express any doubt of the scientific determination of the twenty. No one is ever expected to feel interested in looking up the types for comparison or study after having become familiar with the all-sufficient description of ten lines. This seems to be the opinion of some taxonomists. For they very carefully avoid all mention of the number of types, their identity (see nomenclature of types) and only grudgingly designate the locality from which the types came by the remarkably precise state name. The latter, it is supposed, will give the reader all the ethological information he desires; so that if he wishes to capture specimens of the same species, all he need do is to pack his trunks and hie himself to "Texas" or "Nevada" and pick the species from the

mountain-sides and valleys, from water and land, from trees and grass, or just open his bottles to stop their fall from the heavens. It must be there, for the author said so; he said "Nevada" and this is Nevada.

The following is a scheme for an accessible description:

J——— s———. Not a new species:

1. (a) Sex, usually ♂, and dominant color; size.
   (b) Head: mouthparts, face, eyes, vertex, antennae, occiput, etc.; vestiture, colors, structure, etc.
   (c) Thorax: prothorax, mesothorax, metathorax, structure, vestiture, colors. Legs, their color, structure, vestiture and appendages; etc.
   (d) Abdomen: structure, markings, color, vestiture, appendages, etc.
   (e) Wings: color, markings, vestitute, venation, etc.
2. (a) ♀ and dominant color; size.
   (b, c, d, e) as above. Difference from ♂.
3. Summary of salient characters. Unique characters. Variation.
4. Comparison to related species, position of species.
5. Material: Types, identity (see nomenclature) of types, exact date and locality of capture. How (ethology) and by whom captured.

Of course, this scheme cannot be strictly adhered to in the different orders; it is, however, sufficiently elastic to permit the changes required. What is important in one order, is negligible in another. But the fundamental idea of setting forth by paragraphs or captions the principal parts of a description in successive order, will no doubt be understood.

B. Redescription.—How a redescription should be formed depends on the original description. If the original was carefully drawn, the other may be a summary of the first with possible new points of variation, etc., discovered. Or if, as very often is the case, the original was insufficient, the redescription should be carefully formed; in fact, the author should aim to replace the first with the second description. Even though his name stand not as the sponsor of the species, the task of redescribing is not a thankless one, as need hardly be explained.

Redescriptions are also written for convenience, either as summarizing the knowledge of the species, or, as indeed commendable, to place an otherwise inaccessible description within the reach of the student. Much of what has been said under the preceding caption applies here also and needs no repetition.

C. Generic description.—What is a genus? A classificatory group of plants or animals, embracing one or more species; the primary condition of binary nomenclature; a uninominal used for the lowest phase of the grouping of living forms accepted by naturalists.

What constitutes a genus? A single species or several that, aside of specific differences, have certain morphological features in common, which distinguish them from all other groups of species.

When is a genus valid? When so stated by the sponsor, the nomenclator having noted certain morphological characters, the value of which is recognized by fellow-workers, and who accept this diagnosis upon the given characters; when placed with a monomial (specific) to signify that the species possesses certain distinguishing group characters.

As genera constitute the lowest, but at the same time the most important, phase of grouping, at least some attention should be given to the formation of generic descriptions; especially so in larger contributions, such as monographs and generic summaries and synopses. Generic description is allied to specific description; hence methods ought to be similar.

Some of the essentials of a generic description are the following:

1. That the type species be cited. It should be noted that the type species must be a species then or previously described; else we have merely a nude name.

2. That the characters on which the species is based be given. Although the generic name alone, when coupled with a described species, is recognized as valid by the codes, the systematist will insist that the absence of a generic description is an unfair apprisal on the part of the nomenclator.

3. That these characters be stated concisely; that is, write to the point. Brevity is not conciseness. One may be brief and vague at the same time.

4. That these characters be stated in orderly manner. Especially in larger papers unity of methods is advantageous. If one description begins with the legs, another with head, a third with the venation, etc., study is made difficult. Uniformity of methods facilitates study and progress.

5. That other species belonging to the new genus be listed. While this necessitates thorough study on the part of the nomenclator, it really is his duty. To split up large genera upon characters drawn from a single species is a simple matter. But the nomenclator should verify the stability of his characters by extensive comparison with related species.

6. That other genera be compared, or, at least, the position of the new genus indicated. To describe a new genus of a family already containing twenty or thirty genera and not indicate the position or relations of the newcomer, is not scientific; it denotes carelessness or ignorance.

Not exactly essential, but still of value in extended papers are the following:

7. The etymology of a new name should be explained. This often gives a clue to a character or to the relation of a genus.

8. The distribution of a genus should be cited; namely, whether it is Oriental, Palearctic, Nearctic, etc.

9. The order and family of the genus should be indicated in title or text. This pertains especially to brief papers. As nobody can be familiar with the specialties of all authors or with all generic and family names, this offers an aid in the study and classification of papers.

10. The validity of the generic name should be considered by the author. It is the author's province to do so in the first place. If taxonomists cared to put a little time or expense to the verification of a name, there would be fewer homonymns coined each year.

II. Standards for Colors.

Of all standards these are most needed, since they are most sinned against. That no color standard should exist in a division of zoology, which is of prime importance economically as well as numerically, and where frequently colors are our only tangible guides—unfortunately so—for generic and specific determination, is hardly conceivable. Yet such is the case. After one and a half centuries of entomology, in which the number of described species has been advanced from a few hundred to several hundred thousands, we are utterly lacking of any color standard and are guided in our nomenclature of colors solely by the individual impressions of the taxonomist. That such a basis is absolutely at fault, needs no special asseveration.

The perceptions of most men in regard to colors are extremely crude. (To anyone who may doubt this statement I advise a visit to some artist. One may state to him his impressions of ten different shades of color; and observe then, how often the shade will be misnamed by the amateur as against the professional testimony of the artist. I do not claim a better perception than other men and am found at fault equally as much as others.) In their school days men were taught the tale of three to seven primary colors, and a small trifle of the shades resulting from combinations of the primaries. A little of this they remember through the rest of their lives. And, strange to say, when a man would not use a term or expression to designate an anatomical detail unless he is absolutely certain that it is correct, this same man will unhesitatingly designate colors, when, to say the

least, there is good reason to doubt his exact knowledge of the particular color. I do not say that this is intentional; it results from overconfidence of his particular knowledge. This carelessness arises from the lack of proper standards. Accordingly men are forced to formulate their own standards, which are necessarily at fault It is only through an average or consensus of opinions that standards are reached.

In a desire to be conscientious men often circumscribe a condition when they find their exact knowledge of colors inadequate. This is usually done by the addition of such terms as "pale, light, medium, shining, glabrous, bright, vivid, dark, dull," etc., to the primary color. While this effort is commendable, it offers no more certainly than the mere citation of the primary shade; and the interpretation of the circumscriptive adjective is frequently very liberal.

Probably the most liberty has been taken with the term "fuscous" in our descriptions. This term has been made to designate any darker shading on a light back-ground, beginning with a tinge of the palest yellow against a white or translucent base to a seal or clove brown against any lighter back-ground. "Orange," "yellow," and "green" are others of these liberally interpreted colors. The heart-rending or laughable (as one views it) puzzling of students, who are familiar with exact anatomy but not with the vagaries of taxonomy, when attempting to determine a species from description and to seek conformity between the colors as given by the author and the specimen in hand, affords too well known illustration.

Viewing the matter from the stand-point of my own desultory experiences, the question occurs to me: If at the present time, when the approximate number of described insects amounts to about 300,000 species, identification is difficult, the determination often exhausting the patience of the taxonomist in the vain endeavor to divine the protologist's perceptions of colors; further, this difficulty having encumbered taxonomy with labyrinthine synonymy;—what, then, will be the condition of taxonomy fifty years hence, if we continue with present methods, when species will have increased to approximately 1,000,000?

Happily there is a tendency among our eminent specialists in the last decade to standardize their descriptions as far as colors are concerned. (This is beautifully instanced by Packard in

his later works, such as his monograph of the Lepidopterous family *Notodontidae*). Yet these are so few that their number may be regarded as negligible. That the necessity of color standardization is imperative and that this is well recognized is shown by Dr. J. B. Smith's addition of a plate of colors to his recent "Dictionary of Entomological Terms."

Structural (iridescent) colors are sometimes difficult to define because of the varying hue, according to the angle of refraction and reflection. Yet with reliable color charts these difficulties would be obviated.

Frequently the belief asserts itself that specimens were described in lamp-light. How unsatisfactory and misleading artificial light is taxonomists ought to know only too well. The simple experiment of exposing green, yellow and brown insects, notably shining specimens, successively to gas, electric, acetylene, candle, kerosene and the natural sun-light yields some surprising results.

A color standard need not be an assortment of infinitesimal shadings, gradings, and combinations of the primaries. A representative selection of from thirty to fifty colors is sufficient for all practical purposes.

The fact that detailed comparison of the colors of a specimen to color charts entails some extra labor should not deter taxonomists from making these comparisons. The appreciation and gratitude of their fellow-workers as well as of their followers will be their reward. The dominant color should be stated in all cases. True, the colors of dead insects are rarely quite the same as in life, or those of younger insects the same as those of mature specimens. Yet the fact that colors have faded in death, or that they change with age, is of secondary importance. A description is not based on possibilities, but on tangible concrete actualities. These alone should rule. If there are good reasons for assuming that the colors of the specimen are not representative, this can, and, in fact, should be stated. Having a dominant color as a basis, it is comparatively simple to fix the position, extent, and shade of the other colors an insect may exhibit from further comparison to charts.

The terminology of colors may be somewhat cumbersome. But science is not "belle lettres"; the taxonomist does not consider whether the sentences he reads are syntactically correct or rhetorically rounded, but judges from their contents as to

their value. At that, why a composite terminology? Why not a restricted nomenclature based on a few names with divisions indicated by subnumerals, as red 1, red 2, red 3, etc.. blue 1, blue 2, etc., etc.?

Good works on colors exist, notably Ridgeway's Nomenclature of Colors, as adopted by Ornithologists. (Unfortunately this excellent work is long out of print, and because of its limited edition it is now practically impossible to purchase a copy in the book-market). But for practical purposes a simple chart, as that hand-painted by Frederick Oughton (London), if selected by a representative commission of entomologists, could be manufactured at low expense, which would be easily justified by the demand. This would offer a standard for all times, not to mention the other obvious advantages resulting thereby.

III. Standards of Nomenclature.

A. Generic and Specific Nomenclature.—This is the only sphere where standards already exist. These standards are the codes of zoological nomenclature, such as the International Code of Zoological Nomenclature, the A. O. U. Code of Nomenclature, etc., which are commonly followed by zoologists. If I say "followed by zoologists," the phrase must be given the most general and generous interpretation. Speaking of a class I can say "commonly"; but when speaking of groups of specialists—to say it mildly, many groups use the nomenclature of 1810 instead of 1910. This sounds anomalous, but it is not. For the regulation of nomenclature by codes of universal sanction is comparatively recent, and the commissions are only gradually bringing order into the nomenclatural chaos that existed before their day.

One cannot expect, I suppose, that a specialist on the biological phase of insects should be interested in the "arbitrary, dry" codes of nomenclature. Yet it must be remembered, that taxonomists alone have caused the chaos. Taxonomy is "arbitrary" also. What one man considers a variety, another calls a distinct species; and still another refuses to recognize either opinion. Or are "splitters" and "lumpers" only births of fancy, or memories of the distant past?

The aims of the codes of nomenclature are to make the nomenclature as free and unencumbered as possible. Hence the rules set down for guidance. If taxonomists disdain, or even refuse, to follow these rules, who else should follow them?

B. Anatomical (Morphological) Nomenclature.—Standards for generic and specific nomenclature have been noted. The present issue is of equal significance.

The chief objection that may be stated on this question is indefiniteness. A lesser offense is the scope of the terms; *e. g.*, while by "front" the author may intend to include nasus, epistoma, rhinarium, labium, etc., we, however, know that front means *frons* in the scientific interpretation and nothing else. What the author thinks, we cannot telepathically or by any other means divine.

Each business has its technical nomenclature. No hardware man will hand you a shingling-hammer when you ask for a claw-hammer. To the business man the two terms signify two different things and he will never be so careless as to use the one for the other. Yet among taxonomists we find a continual interchange of terms, such as joint for segment, tarsi for tarsal claws, mouth for labrum or mandibles, abdomen for venter, etc.

When a taxonomist writes "face yellow, abdomen spotted," it is supposed, that he knows what he means. But unfortunately I do not. A specialist, who knows the peculiarities of the score or twenty-five other men working on the same branch of science, will possibly understand what is meant. Not so the individual who attempts to determine a species, less because of special interest, but because of some observation he made on it and which he desires to record in his book of field-notes.

Another idiosyncracy is to use comparative terms for the length or size of any portion of the body, as, for example, "front as wide as the eyes, elytra twice the width of the pronotum, tarsi about two thirds the length of the tibiae, etc." This mode of measurement is miserably uncertain; miserably, because of the misery of the student who attempts to make the same comparisons and cannot see them as the author saw them.

How many men are able to mark the exact middle of a line at a glance? Aside of usual differences in refraction in two eyes, some aberration will be caused by the strain of focusing to the same point. A "mathematical" eye is a virtue that very few people possess. Still more difficult is to find the exact third of a line. What then of paralleling lines, or approaching lines? What of curved lines, irregular lines, etc.? Or is the chapter on "Optical Illusions" as taught in Physics only an illusion?

Bad as color illusions are, mathematical illusions are worse. The chapter on "Optics" ought to form the favorite reading of many taxonomists. A difference of one millimeter on an insect of 20 mm. length is slight; but it makes a considerable difference on an insect of 8 mm. It is a peculair experience to read in a description of a beetle or any other insect "elytra twice the width of the pronotum" and then find by actual measurement that the pronotum is 4 mm. at its widest point while the elytra are 10 mm. or more in length. Similarly with most other comparative measurements. When tested by the micrometer or millimeter scale they will be found considerably aberrant. Hence the urgent advisability to introduce exact measurements instead of the unreliable optical method of comparison.

One standard does exist in anatomical nomenclature, namely the Comstock-Needham nomenclature of wing venation. The merits of this system are undisputed and recognized by all modern systematists. But instead of unreservedly adopting a system the value of which they confirm, taxonomists intermingle the antiquated miscellaneous wing nomenclature with the logical modern terminology. As a result we are continually thrown from one style of naming the veins to the other. This may not be troublesome for the specialist. But if a student is generally interested in entomology, he finds himself in a constant quandary as to the special terminology of each particular order, as they are easily confused; whereas the Comstock-Needham nomenclature was especially designed to obviate this difficulty. It is true, certain orders have certain appendages which it is desirable to retain, *e. g.*, for Neuroptera the thyridium cell and end-forks, bees the subcostal cells, etc. These should be retained, as they are special attributes of the respective order, family or genus. But the fundamental principles of venation, as outlined by the Comstock-Needham nomenclature, are possessed by all orders, viz., costa, subcosta, radius, media, cubitus and anal vein. Why not use them instead of vein 1, 2, 3, 6, 8, 10, etc.? The terminology is simpler, it is less aggravating, it is more logical, and it is an aid to the student and worker.

C. Nomenclature of Types.—Quite as important as specific and anatomical nomenclature is the nomenclature of types. Considerable attention has been given to the latter study in recent years. As the various departments of natural history are dependent mainly upon descriptions for the taxonomic

knowledge of specimens, the types of these descriptions grow in importance as the sum of our knowledge of species increases. The best description is not perfect, but, more often than not, deficient in some important taxonomic character. Hence the need of later systematists to refer to the type as the absolute standard of comparison. A nomenclature of types has accordingly been developed in recent years which is given the same importance as that which taxonomists attach to species nomenclature. While less diversified than the latter, it should become of equal interest to the taxonomist, as it remains for him to apply it.

With the close of the year 1906 we have a series of five primary types and four supplementary types designed to meet the needs of both systematist and type custodian. Some of these designations will possibly be disregarded or even found insufficient; this depends upon the individual, whether he be "splitter" or "lumper."

The first step toward a logical nomenclature of types was made when taxonomists began to set aside one of a series of specimens as the type proper, and to name the remaining specimens cotypes. Too often it had been found that a series which the protologist defined as one species actually represented two or more species. Hence the advisability of naming only one specimen the type and the others differently. The name "cotype", although used so universally, is in such case a misnomer and was finally set aside for the more pertinent and exact "paratype"—to signify specimens of the original series other than the type specimen. As the word "type" is subject to many interpretations according to the combination in which it is used, Schuchert in 1897 devised the word "holotype"—meaning "sole type"—for the single specimen on which a description should be based. The name "cotype", however, was not discarded; its applicability only was limited. "Cotype", in its present interpretation, is properly applicable only in paleontology; for instance, when we have a fossil and its reverse. Another instance, from zoology, would be the following: two flies caught in coitu and not separated in death. If mounted together neither male not female can be called holotype; there is no necessity of singling out one of the specimens, as there can be no doubt of the two belonging together.

The following is a summary of type nomenclature:

A. Primary Types.

1. Holotype (**H. T.**)—A single specimen, or one selected of a series.
2. Allotype (**A. T.**)—A single specimen of the sex not designated by the holotype.
3. Cotype (**S. T.**)—Specimens of the original series when there is no holotype (=syntype).
4. Paratype (**P. T.**)—Specimens of original series when there is a holotype.
5. Morphotype (**M. T.**)—A single specimen of the second form described of a dimorphic sex.
6. Lectotype (**L. T.**)—A cotype chosen after publication as holotype.
7. Chirotype (**X. T.**)—Specimen on which a manuscript name is based.

B. Supplementary Types.

1. Plesiotype (**P. t.**)—Material on which subsequent descriptions or figures are based (=apotype and hypotype).
2. Neotype (**N. t.**)—A specimen from the same locality as the original type described or figured when the original type is lost.
3. Heautotype (**H. t.**)—Specimen identified by the nomenclator or used by him for illustration, but not belonging to original series (=autotype).
4. Plastotype (p. t.)—Plastic reproductions from type specimens. These must be casts. Models not included.

The five prior names (1, 3, 4, 6, 7) for primary types are sufficiently simple and certainly not cumbersome for the systematist. Yet it appears to me that one condition quite as important as the holotype has been overlooked; also a second one, which, if not general, still applies to certain orders of insects.

The first of these is easily apparent, Very many descriptions are based on one sex alone; often several decades pass before the unknown sex is discovered and described. Since this description is of primary interest to taxonomists, the specimen on which this description is based in my estimation also merits a type name; and, what is more, should be classed among the primary types with the holotype. The second case is sexdimorphism, common in a few orders of insects, rare in others, but still of such frequent occurrence that a type name for the dimorphic individual appears advisable. To designate these cases properly I have elsewhere (Bull. Milwaukee Museum, Vol. I, page 10, 1910) suggested the terms "allotype"—the other—for the unknown sex, and "morphotype"—form—for the dimorphic form of a sex.

Allotype designates the sex not represented by the holotype. The allotype need not be described by the protologist (first describer); it can be contained in the original as well as in any subsequent description by other authors. Thus, if the protolog describes only a holotype male, the first female subsequently described is to be called the allotype; and vice versa. Morphotype applies only to the second form of a dimorphic sex. Here also the date when and the author by whom described are immaterial. (As the first form of a dimorphic sex will be represented in the holotype or allotype, there may be some doubt as to the advisability of classing morphotypes among primary types. However, as both forms of a dimorphic sex are of equal importance to taxonomists I have placed morphotype in a position similar to the holotype and allotype.)

Thus far few others than cataloguers have made use of the type-terminology here outlined. In fact, most of the terms were originated by them, since the thorough acquaintance with their subject gained by the compilation of catalogues has made them more susceptible to the various needs of taxonomy. As all of these terms are broad and permit of great latitude in interpretation and application, the systematist ought not hesitate to apply them. Past laxity in the treatment of types, and also in their preservation, has resulted in infinite confusion and has helped to increase synonymy beyond all reasonable bounds, so that in some orders the synonyms average 1.5 to each valid species.

IV. Standards for Keys (Tables) of Genera and Species.

1. ♂ with appendage to hind tibia.....2
   ♂ without appendage.....4
2. ♀ with abdomen tufted.....3
   ♀ with abdomen untufted.....Kilimanjaro
3. Vein 6 usually curved in ♂, ♀ variable.....Popocatepetl
   Vein 6 usually straight ..... Aconcagua
4. ♀ with abdomen untufted.....5
   ♀ with abdomen tufted .....Matterhorn
5. Vein 6 curved ..... Elias
   Vein 6 curved at end in ♂..... Everest

I defy anybody to reduce a specimen to its proper genus with a key of the foregoing type. Unfortunately, only too many of that sort exist and new ones are continually fashioned.

A genus is the primary condition of taxonomy, and the use of secondary sexual characters for generic definition is an outrage; an offense, which should not be condoned. Some of the

best taxonomists have placed their work in a questionable light by means of unsatisfactory tables like that given above. The only recourse in such cases is the original description, which is by no means such a simple proceeding as would appear on the face of it, as it often means a long, tedious search through many volumes.

One may call the aid of the extended generic description, but the purpose of the key is to summarize what differences exist between genera. Tables are meant to be short-cuts through taxonomy; but I might as well try to run an engine on a railway which has one track alternately on each side of the ties, as determine a specimen from many generic tables. The use of geographical names in the key above is pertinent. It is just as difficult to climb those mountains as to determine specimens from some keys. Tables of the style outlined cause loss of time, besides loss of temper. We are all human; and a scientist is not always the "dry, imperturbable fossil" the joke-antiquarians would have us believe.

Among species tables we see many of similar nature. Yet here vagueness is excusable, while for an unsatisfactory genus table no valid excuses can be made. If the relations between two genera become too intimate, if distinctions fail—then the genera merge.

Sexual characters are often the only ones that can be reliably applied in specific keys, and their use will be questioned by no one familiar with the difficulties of specific determination. Errors are possible everywhere, but they are offset by good work in other parts of the paper. Most often they result from a misconception of the specific value of certain characters. The aim, however, to compile a table of practical value will be easily apparent.

Many of the difficulties of specific keys could be obviated by more care in the explanation of the essential characters used, their individuality, their variation, and their relation to others. But is there an excuse for the use of such terms as "larger species," "smaller species," "more slender," "more robust," and the like, in tables without in any way defining the limits of the terms? It is with feelings diametrically opposed to pleasure that I plod through a table of, say, 25 species, along lines indicated by "larger species," and "smaller species." What does the author mean thereby, I wonder? At which

size does he draw the line? My specimen is of moderate size and might be referred to either group. Therefore, is bulk the author's criterion? Or is length? Or width? Or odor?

There are plenty of good, workable tables that will serve as models. An ideal table that would permit of "hard and fast" lines of division for species is, of course, impossible. But much could be done toward improvement by the elimination of indefinite terms from specific tables and sexual characters from generic tables.

V. Standards for Indices.

Indices are the bane of scientific works. While their purpose is to facilitate reference to, and study of the contents of a volume, it is rarely, indeed, that they achieve their purpose, because of their general insufficiency. Beginning with ordinary check-lists, bibliographies, travels, monographs, etc, taxonomic works are most often poorly equipped as regards indices.

It is impossible for any man to know all the species and genera of the average order. It is a fact, however, that just those publications which are greatest in volume and importance (taxonomical, ethological and otherwise) are the most poorly indexed. Some authors cite only genera in the index. Others feel that such method is insufficient and append the names of the species under the genera. While that is an improvement, it offers little aid to the student not familiar with the particular order.

In this age of books, when it is possible to distinguish genera, species, synonyms, etc., each by various styles, sizes and impressions of types, the antiquated system of indices, as above referred to, seems inconceivable. The trouble lies—so it seems to me—in the fact that authors seem to confound the index with a table of contents.

To quote, "an index is a pilot through strange seas of thought. A book without an index is like a ship without a rudder." Continuing the simile—a book of entomology with generic index only is like an ocean-steamer with a canoe-rudder; and an index with the species names under the genera is like a ship with the rudder at its side.

I need hardly assert that it is those books which are freely and carefully indexed that are most referred to. I feel much as the gentleman who said to me: "A scientific writer who does not care to make a complete and usable index to his works,

should be prevented from writing at all! At the bottom of every insufficient index is not carelessness, but downright laziness!"

To set the standard for indices is not very difficult; but the standard varies with the contents of books and papers. Here is the criterion: Since the aim of an index is to make the contents of a volume accessible to the reader, it should be so constructed that it will permit access to the greatest possible number of references in the least possible time. In other words an index is a medium of saving time. Hence an index should not be merely a carelessly jumbled summary of the contents, but a carefully arranged alphabetic list of all names, facts and captions in the volume. This includes technical as well as popular names, generic as well as specific names.

There is such a thing as over-indexing. The author must use his judgment as to the amount of detail he desires to index. Also, unnecessary repetition should be avoided. One fact, however, is patent; that if the author wishes to see his work considered at all as a work of reference, he must supply it with a good index. I, for one, do not care to use poorly indexed books, and consult such as rarely as possible. To say the truth, I consider it a personal affront, when upon purchasing a book, I find myself maltreated to several hundred pages of facts and names, and a two-page index. The author has no cause to treat his readers as if their brains were ware-houses; that they need but read his book and file away the contents together with the exact page number, etc., for future reference. By purchasing and reading a book I am doing the author a twofold service. And if I remember some of the statements and quote the book as an authority, the acme of the author's expectations is then reached. More he has no right to demand. But a starved index is inimical to progress, since few men will care to quote when they are unable to find the passages from an insufficient index.

When is an index desirable? One friend has stated this succinctly: "Any taxonomic paper citing more than fifty names should have an index of its own." This seems reasonable to me. An index of fifty names, run in two columns, eight point on a ten point base, would occupy less than the ordinary four by seven page of our journals. Because of the practice of societies and institutions to send reprints to an author for pri-

vate distribution, this special index seems more than justified; unless the author expects his associates to supply the index privately. But this is expecting too much. Take, for instance, some of our well-known entomologists, who receive hundreds of reprints in a year, among them contributions exceeding 100 pages. It is astonishing, how few of these larger papers are supplied with an index at all; at that, the indices are mostly of the Spartan type. Should these men undertake the necessary clerical work and compile the missing indices? True, many of these men keep card-indices of their specialties. But what of workers on more than one branch of entomology, or zoology? To keep card-catalogues—hence general indices— of their widely distributed interests would necessitate the employment of a clerk throughout the year.

I close with the classic from Pope, "He who knows how to prepare a good index, holds the eel of science by the tail."

VI. Standards for Titles.

In logical order the title should have been treated first. But since the title is usually the last thing written by an author for his contribution, so let its place be among the last in the order of standards.

Take any entomological journal in hand and glance over the titles of papers. Many of these will sound much like the following examples: "A Revision of the Genus Popocatepetl; Some New Species of Orizaba; A New Aconcagua; A New Variation and the Life History of Kilimanjaro alta; etc." Occasionally one meets a title like the following: "A New Genus and Species of the Family Sierra"; and indeed a rarity is "New Species of the Order Andes."

In North America alone there are about 70,000 described species of insects, distributed in approximately 8,000 genera (probably more). Nevertheless, everyone is, as a matter of course, expected to know immediately from the lucid "Genus Popocatepetl" just where the genus belongs, to what family, to which order. Everyone is expected to be familiar with all of the 8,000 genera and to have no difficulty at all in placing the genus revised or enlarged, as indicated by the title. And even considering that there are about 500,000 specific and 80,000 generic names in zoology, "Popocatepetl" is too important not to be as well known as "pater" and "mater."

Especially in taxonomic entomology the saying holds good: "Familiarity breeds contempt"—for others. Some taxonomists appear to become so obsessed with their particular specialty that other orders or families of insects do not exist for them. There are 18 other orders after Comstock, 30 others after Handlirsch (restricted to *Pterygogenea*—winged insects); yet these are of little importance beyond the fact that they exist and that some foolish people bother about them. So taxonomists of a certain type would have us believe. We are lucky, indeed, if with indignant compassion they will cite the family in which the order occurs; indignant, because "those barbarians" do not happen to take any special interest in their particular branch.

Let us go a step farther. There are eighty-two families in the order *Coleoptera*, sixty-one in *Diptera*, about seventy-five in *Lepidoptera*, about seventy in *Hymenoptera*, not to speak of *Hemiptera*, *Neuroptera*, *Pseudoneuroptera*, and other orders. A conservative estimate would show over four hundred families of insects in North America alone, distributed in nineteen (Comstock) or thirty-one (Handlirsch) orders. Most of these families average three to four subfamilies to each family, and two tribes to each subfamily. Figuring on this basis there are 1200 subfamilies and 2400 tribes of insects. And this for North American insects only! What of the orders, the families, the subfamilies, the tribes, the genera, of fishes, of mollusks, of birds, of mammals, of crustaceans, etc. in North America? What of their number in the entire world? Not all our articles are confined to a single fauna. The Central and South American faunas are beginning to be explored more thoroughly, as shown by the ever increasing number of articles upon the regions named.

And yet, on an average but six out of twenty titles cite the family, and but one of twenty the order. Of course, the fact that the journal is specially devoted to entomology, gives me a clue to the position of the genus; accordingly I know that the paper is an entomological paper, but that is all. But what of journals dealing with natural history in general? How can I know from the title whether the genus belongs to botany or to zoology or paleonotology, whether it is a paper on insects or canaries, on mollusks or angle-worms?

An hour spent in a scientific library in the classification of articles would be an educative influence for all those who neglect

the mention of family and order in their articles, The difficulties they would meet—such as antiquated catalogues, under-indexed catalogues, or, as in some cases, the entire lack of catalogues—would forever cure them of this apparently trifling but nevertheless momentous negligence. Even when there are good catalogues at hand, it is a complex proposition to place a genus. For the terminology of some orders, such as *Diptera*, and *Hymenoptera*, *Coleoptera and Hemiptera*, etc., is, in part, alike; the necessary consultation of both text and catalogue in such cases causes an irksome and avoidable loss of time.

The solution of all troubles is so simple, so obvious—in fact, it is inherent in the subject—that it seems strange why taxonomists have not adopted the simple means. But one entomologist is known to me who in all of his papers inserts the order name in his titles. That is the solution: Insert the order of the insect, bird, mammal, or whatever-it-be behind the genus and family name in the title. This holds good also for morphological, ethological and other papers as well as for those dealing solely with taxonomy.

VII. Standards for Reprints.

This chapter does not properly belong in this consideration. But since reprints form an important part of the specialists' literature, a few words on the topic may be of interest.

Sometimes I receive reprints of articles published by "Enigma" University; that is a tangible fact. The paging of the reprint is the same as originally published; that is another tangible fact. But I look in vain from page to page in the endeavor to discover the number or year of the volume, the month of publication, etc. That editor who arranged the reprint of an article sent me, published in nineteen-something on pages 260–290 of a certain periodical, yet paged the separate 1–30; and carefully effaced all reference to the name of the publication, the year or number of the volume, the year and month of publication; that editor, I say, deserves no honorary mention. After guessing at the probable publications in which the article might have appeared, I looked over the recent volumes of many and ultimately succeeded in finding the exact place, page and time of publication. I owe that editor thanks, since through him I was led to other articles of high interest; but I spent an entire evening in trying to find out "What's which" in the reprint. To be fully consistent, the editor should have effaced the title of the article itself.

To be sure, this was an extreme case. Yet that in these "enlightened" days, after years of discussions, protests and recommendations, there should be men who retain the benighted idea that it is preferable to change the paging of reprints from the original—this seems hardly conceivable. Why the change at all? No advantage is gained thereby. On the contrary, it is a disadvantage for workers who are not constantly in touch with all the leading centers of scientific work and who have no large scientific library at their elbows. For these it results in tedious correspondence, and this most often when there is little time to be spared for these irksome labors.

One lucid individual went to another extreme. The travels of a certain explorer, together with the scientific results of his collections, as monographed by various specialists, were published in a large scientific journal. As all of these contributions were finally to be collected in a separate volume, and as the paging of this volume would be just as important for reference as that of the journal, the editor thought of a "happy" solution of all difficulties. Namely, the original paging of the contribution as it appeared in the journal was retained for the reprint; the future paging of the volume was also put in; and to meet all contingencies the reprint was given a special paging of 1—50 or other. Unfortunately, this genius forgot to note which was which, so that, as the printer's folio number and the publisher's file number are at the bottom of each page besides the three numbers above, I now have my choice between five numbers for page reference.

As a rule reprints do not suffer from surplus information as in the preceding case; they usually lack part of the necessary information. This lack in most cases is the absence of the volume number (or the year of the volume) from the reprint, or the year of publication, or both. Sometimes the two are given, but the name of the publication is nowhere indicated. The benign opinion that every scientific worker is familiar with the size of the volumes, the style of composition and the issues of "the four-hundred" leading scientific publications,—this opinion is, of course, founded on long experience and hence must be considered sound. If I receive a reprint that contains the year and number of the volume, but not the title of the publication itself, it is, therefore, a simple proposition to locate the correct journal from the size of the page and the style of com-

position, as there are only about three hundred others among the "four hundred" that resemble it.

Often the title of the journal is present and the number of the volume given, but not the year of the volume. The latter is omitted because it is a matter of common knowledge that the institution or society began its journal way back in the forties and that a new series is begun with each score of years; so that the tale, "Reprinted from the Enigmatical Journal, Series 4, Volume 17" will tell me all that is necessary to be told. From the number of the volume I ought to infer the year of the volume and if I am too much of an "ignoramus" as not to know such a monumental fact as the year a certain society or institution was founded,—well, then "look it up!"

Similarly, if I read 1906 on a reprint just received, I am to know intuitively that that means the year of the volume, not the year of publication; that the contribution had been in the hands of the editor since 1905, but owing to the press of legislative matters on the state printer could not be published until 1910.

It appears ridiculous that a matter intrinsically so simple, and extrinsically of such vital importance as the correct marking of reprints should be so carelessly treated. Or is there really a living editor who would consider the puny additional (?) expense of the line on the reprint giving all the needed information? Penny wise, pound foolish. Can a simpler solution be found than "Reprinted from the Ecstatic Journal, Series 6, Volume 14, pages 28–67, 1910 (Publ. May, 1910)"?

Conclusion.

The scope of matters that are left to our imagination, divinition and intuition by scientific papers is monumental. A catalogue of merchandise that does not describe the ware and state its prices would be flung aside instantly. Yet for science anything, no matter how poorly constructed, how poorly presented, should be acceptable. Science should lead the world. But if science in general cannot apply more logic to its methods than taxonomists apply to taxonomy, its leadership will be short-lived. This may be a harsh and pessimistic view; but I believe that I do not stand alone in this attitude.

Again referring to the merchandise simile—imagine to yourself a catalogue of merchandise, say furniture, that would not bear the proper legend on the cover; further, that the pages

contained nothing else but names of furniture—no illustrations of the same, no measurements, no prices quoted;—imagine the action of the man receiving it! Certainly no other place than the paper-basket would be accorded it. And certainly many of the articles of our journals are little better as far as usable information is concerned than the furniture catalogue just referred to. Is it with reverence that we remember such names as Smith and Walker of British Museum fame? And yet some systematists appear to have chosen them as patrons and models for imitation. They succeed only too well in imitating them, and occasionally outdistance them.

One may say, these are all minor matters. That is true. But their aggregate forms an imposing array. One drop in a cup will not make it acrid; but a number of drops will change it into a cup of bitterness. So with entomology. One little carelessness does not amount to much; but many will fill even the most ardent student with feeling akin to disgust.

Science is no longer in its infancy and we have a right to demand advanced methods of work. The desire for improvement is innate to all men. I have never heard of a writer (at least in science) who was well satisfied with what he had written. Literary critics say, "An author is his favorite reader"; but self-satisfaction is short-lived, more so in science than elsewhere. Hence the attitude of scientific workers toward their work may be defined as "a minimum of self-conceit with a maximum of scruples." Writers do not confess these qualms of the intellectual conscience to the public, but reserve them for some private interchange of confidences. Unfortunately, the ratio of these qualms decreases, not inversely, but in the same ratio that the system and methodical effort of the worker decreases; so that the most conscientious workers are usually most diffident as regards their own work (all the more, as those contributions requiring the greatest amount of labor and time generally show the least for it), while the careless workers have few misgivings of their efforts. I have an inkling that some day to come a contribution will have to be passed upon by a commission of scientists (like so many examination papers) before they are declared acceptable to science.

Cooperation and centralization (to a certain extent) are desirable. There ought, in fact, to be a scientific clearing house somewhere in this beautiful world, and I hope that it will be achieved some day.